

pages, and contains merely a few illustrations of the capacity inherent in music of modulating the pleasant sensation it produces in the mind of man in a number of various ways. An appendix treats of the pleasure man derives from the aspect of colours, certain forms, and the beauty of the human body.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

On the Spectrum of Brorsen's Comet

THE observations of Prof. Young on the present appearance of the spectrum of Brorsen's comet are of great interest, from the circumstance to which he refers in his letter in *NATURE*, vol. xix. p. 559, namely, that in 1868 I found the positions of the three bright bands of this comet not to agree with those of other comets which I showed to be coincident with the bright bands in the spectrum of flames containing carbon.

The care I bestowed upon the determination of the apparently anomalous character of the spectrum of Brorsen's comet in 1868 gives me great confidence in its approximate accuracy. I wish now to call attention to the fact that a spectrum apparently essentially similar to the peculiar one exhibited by Brorsen's comet in 1868, was observed at Dunecht by Lord Lindsay in the case of Comet C 1877 (Borelly's). It is remarkable that another comet, Comet B (Winnecke's) 1877, observed by Lord Lindsay on the same evening (May 6) presented the ordinary cometary spectrum.

Lord Lindsay's diagram in the *Monthly Notices R.A.S.* (vol. xxviii. p. 431) of these two spectra agrees as nearly as can be expected in such observations with my diagram in the *Phil. Trans.*, 1868 (Pl. xxxiii.), contrasting the spectrum of Comet B, 1868, with that of Brorsen's comet.

It may be accepted, therefore, as beyond doubt that the unusual form of spectrum of Brorsen's comet in 1868 is occasionally presented by comets. The great interest of Prof. Young's observation lies in the information which it gives us that the same comet may present on one occasion one spectrum, and on another the other spectrum.

I regret that the special arrangement of my telescope for photographic work does not permit me to observe the spectrum of Brorsen's comet at its present appearance.

Upper Tulse Hill

WILLIAM HUGGINS

The Migration of Birds

IN *NATURE*, vol. xix. p. 433, there is a notice of my paper "Ueber das Wandern der Vögel," to which I have somewhat to reply.

However agreeable it is to me that my views should be communicated to your readers, and however little I object to their being submitted to rigorous criticism, I must still also desire that this criticism be fair.

I believe it is due to differences of national customs that your reviewer has not quite satisfied this desire. We make, perhaps, in Germany a sharper distinction between a *scientific* treatise and a *popular* work than in England. Of the latter we do not require that it bring forth what is *new*, but only that it should give what it has to give in a clear and easily intelligible manner. Nor do we require completeness of such a work, or even a criticism of the scientific works on which it is based; indeed, it is generally left to the author how far to cite his sources of information and how far not. In the *scientific* treatise it is quite otherwise; here only that is of value which is *new*; the theme must be treated *exhaustively*; the sources must always be named and dealt with critically, &c.

Now my publication is a lecture, which was delivered before a company of educated ladies and gentlemen, and so before mere laics, and a year and a half afterwards was printed in Virchow and Holtzendorff's Collection of Popular Lectures. It thus belongs unquestionably to the category of popular writings.

For this reason your severe critic had no occasion to point out that in my lecture there is much that had been long known, that sources are named but rarely, and that no scientific criticism is exercised. That is quite a matter of course in a popular work, at least in Germany. Mr. Newton would have had much better right to feel surprised that even any new ideas were contained in it.

My original aim in this lecture was merely to make my hearers acquainted with the new facts and views on the migration of birds, as they have been established by Wallace, Middendorff, and especially by Palmén. As I followed the new facts theoretically to their consequences, there arose perhaps some new ideas, which I should be glad to find verified in the future.

It is further a matter of course that, notwithstanding the popular form of my work, I stand by all that I have said; but I must protest against being made responsible for what I have not said!

Thus, e.g., I have nowhere said that I hold Palmén's routes of flight for "absolute truths." I am rather quite of Mr. Newton's opinion, that these routes are merely *inferred*, not *directly* observed, and therefore that they are to a certain extent "conjectural." In this sense, however, the routes of birds must ever remain comparatively "conjectural," unless one were to follow the birds in a balloon. But while "conjectural," Palmén's routes are yet inferred by a purely scientific method, and I doubt not that most of them will in the main be confirmed by further observations. Precisely in the application of this method lies Palmén's great merit, and it is only to be hoped that ornithologists will follow further in his footsteps, and correct his mistakes by accumulation of new facts. That Palmén's routes contain some errors I do not doubt; I should rather wonder if it were not so.

Little, however, comes of this with reference to the questions which are treated with special fulness in my lecture, *the origination of the instinct of migration, and the powers by which the bird reaches its distant goal*.

I have, further, nowhere said that birds fly over the sea at a height of 20,000 feet, but have merely cited the fact that birds have been seen at such height; with reference, of course, to explanation of their flight over the sea. I believe that birds, in flight over the sea, do not close their eyes, but exercise their keen eyesight as far as possible. Therewith, however, it is not said (as Mr. Newton imputes to me) that in all flights over the sea they always keep the land in sight.

I desist from adducing further misunderstandings by Mr. Newton, and come to what I have actually said and am minded to maintain.

In agreement with Palmén, I have expressed the opinion, that *migratory birds have no special sixth sense*, as Middendorff has assumed, but that they find their way only with the help of their ordinary five senses.

Mr. Newton seems to be of a different opinion. He does not say, indeed, whether he agrees with Middendorff, but he brings forward observations which appear incapable of harmony with my view.

First, there appear in New Zealand two species of cuckoo (*Chrysococcyx lucidus* and *Eudynamis taitensis*) which regularly fly some 1,000 miles' distance over the ocean. I believe with Mr. Newton that the birds cannot fly so high as to see at once New Zealand and the Norfolk or Kermadec Islands, though on the former is a hill of 1,000 feet. Likewise I will accept the case of *Charadrius plumialis* as a regular guest of the Bermuda Islands, and a doubtful *Charadrius* species as regular guest of the Sandwich Islands. All these observations are, indeed, still very imperfect, inasmuch as it is not known whence the birds come nor whither they go; but so much seems certain, that they do regularly fly over large stretches of ocean in which are almost no islands or rocks, and which are so great that they must of course also fly by night.

What then? Are we therefore compelled to make the assumption, with Middendorff, of a sixth sense, which informs the bird which direction is north? Is there no simpler explanation of the fact? Obviously, we should only be warranted in accepting such a purely hypothetical sense, if it were clearly proved, that we could never get to understand the facts without it.

The question had already occupied me, before I knew of Mr. Newton's examples. I omitted it in my lecture, because it seemed to lead me further into the region of hypotheses than I considered I could answer for before my audience.

I do not believe that we are necessitated by the far sea flight of birds, to assume a sixth sense. Is it not conceivable that birds are capable of keeping exactly the same direction of flight for many hours together, and so to fly somewhat like a shot ball or a steamship with rudder bound fast? From the physiological side, it might of course be objected that a very slight difference in the strength of the right and left wing-beats must cause a deflection from the original course, just as in the case of rowing without a steersman, a constant control by sight is necessary, if the right direction is not to be lost. To this might be replied, however, that birds are so accomplished in flight, and that we may assume they have an *extremely fine muscular sense*. Besides, they migrate mostly in company, and an error in flight of one bird will be easily corrected by the others.

But how do they hit the direction in flying away from the coast? They must be able to exactly measure the angle at which they ought to leave the land. Therein, of course, a quite small error would involve great deflections from the proper course, *but do we know that this does not actually occur often enough? and may it not be supposed that in many cases corrections are made in the flight, as soon as any point of orientation again emerges in the circle of vision?* So much we at least know, that even on land birds wander not infrequently. And it is at least not demonstrated in any one of the cases cited by Mr. Newton, that the birds referred to appeared on those islands every year, nearly at the same time and in the same number.

Mr. Newton adduces a second series of "facts" which seem to be against the sufficiency of the five senses; *but are these really facts?*

The young, scarcely three months old, of many of our birds, are said to pursue their flight southwards in autumn *alone*. Is that certain? and have we not here, perhaps, a too ready deduction of general rules from a few well-observed cases? Mr. Newton even says: "This seems to happen with nearly all the accipitres," &c. He quotes a letter from M. Gätke, stating that in July "Young starlings pass over Heligoland by hundreds of thousands *without a single old bird accompanying them*." I confess that I cannot regard this as a *fact*, but as a more or less probable *conjecture*; for M. Gätke, though an excellent ornithologist, could not possibly have inspected a hundredth part of these "hundreds of thousands" of starlings flying about.

I do not mean to assert that these or the other data are false; they may well be correct. I merely hold that we must guard against building far-reaching theoretical inferences on observations the general validity of which is not in the least demonstrated.

But even supposing that all these data are correct; further, supposing it certain, that these young birds, which go forth alone, also actually find the route of the species with the same certainty as if they had known it long before, would these facts be explained by the supposition of a magnetic sense? I think not. For in that case, what must have been born with the young bird? Merely this magnetic sense? *i.e.*, the power of directly perceiving external direction in its own body? By no means. There must also be born with the young bird the consciousness of what angle to the magnetic meridian it must shape its flight at.

But much more than this. It has been long known that birds, so long as they are migrating over land, frequently alter their direction; hence, supposing the young bird to be guided by a magnetic sense, there must be born with it the tendency to fly (say) twenty miles at an angle of 45° to the magnetic meridian, then 100 miles at an angle of 27° , and so on. That this is a physiological absurdity, no one would deny.

For these reasons I hold that a *special sense for direction does not exist in birds*, and that the phenomena of migration, however wonderful they appear, yet cannot ultimately depend on magic (*Zauberei*), and in this Mr. Newton no doubt agrees with me. Hence, nothing remains but to try to explain these phenomena by the known physical and mental properties of birds; *for there is no third course*.

I shall be rejoiced if Mr. Newton succeed with this better than I.

AUGUST WEISMANN

Freiburg im Breisgau, March 31

THE editor having afforded me the opportunity of seeing the foregoing remarks, it will, perhaps, be convenient to the readers of NATURE that I should here add the comments I have to make upon them.

I deeply regret if my criticism of Dr. Weismann's treatise or

lecture be open to the charge of unfairness. I had no wish to misrepresent him, and I cannot see that I have been guilty of such an act—indeed, the wide publication of his theory would render any attempt to do so futile. As to his acceptance of Dr. Palmén's conjecture for "absolute truths," I must urge that he took no exception to any of them, while, in the case of his Bernacle or Brent Goose, he especially adopted (p. 27) that route X which I had particular reason to consider unfounded. I did not assert that Dr. Weismann spoke of birds flying over the sea at the height of 20,000 feet, though there seems no reason why some might not, if they can do so over the land; nor did I impute to him that they always keep land in sight. I had no need to declare my disbelief in Dr. von Middendorff's magnetic hypothesis, for I never met with any man that held it. I had spoken of it already elsewhere (*Encycl. Brit.* Ed. 9, iii., p. 769), and I considered it had been set at rest for ever by Prof. Baird in the article I cited. In like manner it seemed useless to disclaim any belief in the possession by birds of a "sixth sense" which is not common to ourselves and other animals. My only object was to show that Dr. Weismann's theory was inconsistent with certain facts, and nothing he has since adduced makes me think it otherwise. As to some of these "facts" he is incredulous, and I have no fault to find with his caution in this respect, but I am sure that the more he investigates them, the less he will be inclined to demur to them. I shall leave to the ornithologists of New Zealand the defence of those that relate to their cuckoos. Dr. Weismann will find in Mr. Jones's "Naturalist in Bermuda" (London, 1859) more than enough to justify my allegations in regard to the passage of *Charadrius virginicus* (not *bluvialis*) over those islands; indeed it has long been notorious; and as to the plovers of the Sandwich group, I have not only to thank Capt. Long, R.N., for his confirmation (*supra*, p. 460) of my statements, but also Prof. George Forbes, who kindly informs me that when there, on the occasion of the transit of Venus, he shot scores of these birds, and that his friend Capt. Cator, R.N., of H.M.S. *Scout*, having sailed thence, was overtaken in mid-ocean by them, flying in a direct line for Vancouver's Island, on arriving at which he found they had already reached it. Concerning the "facts" relating to some young birds preceding their parents in migration, the more inquiries I make of well-placed observers the more satisfactory are the answers. For want of space I cannot here give the details, but I may just say that Mr. Cordeaux, who has been for many years a watchful observer of migratory birds on the Lincolnshire coast, has named to me nine species of *Limicola*, of which he has personally assured himself that the young migrate apart from, and invariably arrive earlier than, the old—thus fully bearing out Temminck's assertion, made nearly forty years ago. The case of our cuckoos, which I cited, is incontestable, and M. Gätke, I doubt not, will satisfy any scruples about his starlings in that book which we are expecting from his hands.

I will also take this opportunity of replying to Mr. Pringle's note (*supra*, p. 481). My chief reason for not referring to the matter of temperature was that we know too little of the power of birds to resist extreme cold to depend much upon it, and I thought I would not take up room by bringing in that question. Doubtless there is something in what he says touching the loom of land, but I fail to see how it will help very far, and especially in nocturnal flights.

ALFRED NEWTON

Magdalene College, Cambridge, April 20

Colour in Nature

I WISH to offer a few remarks upon Mr. Wallace's kind and appreciative review of my work on the "Colour-Sense" in NATURE, vol. xix. p. 501. Mr. Wallace attributes to me "many errors" and inaccuracy as to matters of fact; but I do not think the instances he alleges are sufficient to justify the statement. Had I said in every case what Mr. Wallace makes me say, I should, doubtless, have been misrepresenting facts; but it seems to me that in most of the passages to which he refers he has slightly misconceived my meaning. I should not attempt to oppose so distinguished a naturalist on points of biological inference, but I venture to defend the accuracy of my statements of fact.

1. "*Scissirostrum Pagei* does not 'belong to a family generally dull,' while it is itself decidedly dull-coloured." The first statement will be correct if we place *Scissirostrum* among the brilliant starlings; but Mr. Wallace himself, following Prince